

# Microfinance and Home Improvement: Using Retrospective Panel Data to Measure Program Effects on Fundamental Events

*JEL Classifications: O12, O16, C21*

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March 2007

Abstract: In this paper we present a methodology from which researchers may be able to estimate development program effects from a single post-treatment survey within an institution's own client base. In our methodology we create a retrospective panel data set based on "fundamental" events in the history of surveyed households, events that are discrete, unforgettable, and important to welfare. Our application of the methodology examines historical dwelling improvements among 218 Guatemalan households with access to microfinance: the construction of cement block walls to replace adobe walls, cement floors to replace dirt floors, tiled roofs to replace corrugated iron roofs, and land purchases. We carry out estimations using village- and year-level fixed effects to analyze the timing of these home improvements relative to the timing of microfinance access and treatment. Our paper presents a series of tests, diagnostics, and corrections within this methodology to account for supply-side endogeneity in the rollout of a program and some types of demand-side endogeneity in the uptake of a treatment. In our estimations, we find microfinance associated with modest increases in the probability of some dwelling improvements.

The authors wish to thank Craig McIntosh for extensive input on this paper along with Alessandra Cassar, Philip Fanchon, Michael Jonas, Dean Karlan, David I. Levine, David McKenzie, Ted Miguel, Jeff Nugent, Jean-Philippe Plateau, Elizabeth Sadoulet, John Strauss, Xavier Giné, and seminar participants at the World Bank, USAID, the University of Rome, the University of Southern California, and the 2007 Pacific Conference for Development Economics at UC Davis. We also wish to thank Adam Gorski and Karina Vargas for excellent research assistance and data collection. Grant funding from BASIS/USAID, the McCarthy Foundation, and the Jesuit Foundation is gratefully acknowledged.

# 1. Introduction

There has been much written recently by development economists about the need for rigorous and systematic appraisal of the effectiveness of anti-poverty programs in developing countries (for example, Armendáriz de Aghion and Morduch, 2005; Easterly, 2006). Yet researchers and practitioners seeking to ascertain the true impact of development programs face a daunting task. Accurately measuring program impacts is both time-consuming and costly, especially for small institutions that seek accurate measures of their effectiveness. Moreover, many institutions would like to evaluate the effectiveness of their programs after implementation, creating problems with the establishment of baseline surveys, control groups, and other means of identification. These obstacles have contributed to a dearth of rigorous analysis in trying to ascertain bona fide impacts of many types of development programs, including the most popular and widespread today, microfinance.

In this paper we present a methodology for ascertaining welfare changes brought about by development programs that can be employed *ex-post* to program implementation, and may be applicable in a variety of contexts. Our methodology, a Retrospective Analysis of Fundamental Events Contiguous to Treatment (RETRAFECT), uses a single cross-sectional survey to create a retrospective panel data set based on fundamental events in the history of households. We define fundamental events as those events in a household's history that are discrete, unforgettable, and important to household welfare. Analyzing the timing of these events within a window around the timing of treatment allows for statistical tests based on changes in household welfare variables occurring after treatment. In this way our methodology borrows from "event studies" undertaken in the finance literature, where the effect of events such as mergers and acquisitions are observed on stock prices.<sup>1</sup> Here, however, instead of examining

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<sup>1</sup> For an excellent review of event studies in finance see MacKinlay (1997).

changes in equity prices within a time window surrounding a merger, we examine the *probability* of fundamental events within the time window surrounding a treatment.

In presenting the methodology, we suggest a number of diagnostics which allow researchers to test for whether the rollout of the program across communities was exogenous to impact variables, and offer a number of caveats in the interpretation of results. These caveats are particularly important when households have some choice over if and when they take up a treatment (such as our current application to microfinance) but are less of a concern with interventions for which uptake is instantaneous and all-inclusive within a community. Indeed, we argue that the RETRAFECTION methodology can be applied in a much more straightforward way to studying the impact of community-wide treatments such as the introduction of fresh water systems, electrification, and roads.

Nevertheless, we present the methodology in this more challenging context in order to outline a series of steps researchers may take in order to obtain estimates of post-program changes even when demand for a treatment involves household choice. Indeed we believe the approach is quite general, and applicable to a wide variety of contexts. The main advantages of the RETRAFECTION methodology are 1) that it allows for an estimation of the dynamics of household welfare changes over time surrounding the program; 2) that these estimations can be carried out from a single survey within an institution's client base; and 3) that the survey can be carried out *ex post* to treatment, where treatment has occurred among different populations at different times.

We show that if survey data passes a given set of diagnostics, or in the case that corrections can be made for certain types of endogeneity, one can employ the RETRAFECTION methodology to estimate a type of intention-to-treat effect (ITE). Because a sample may consist entirely of program participants, we call this the *intention to treat effect on the eventually treated*. In cases where a RETRAFECTION methodology is used where uptake of the treatment affects an entire village population (such as a road or water system), our estimations converge

to the standard ITE and Treatment Effect on the Treated (TET). From here we suggest a set of diagnostics which helps to check for certain types of demand-side endogeneity regarding treatment take-up by households in order to estimate what we call a *take-up effect* and the dynamics of the take-up effect on households.

We apply this methodology to studying the effects of a microfinance program in rural Guatemala, studying changes in the probability of major dwelling improvements, upgrades of walls, roofs, floors, the installation of indoor toilets, and the purchase of new land, using a linear probability estimator that incorporates village and year-level fixed-effects. In short, what we find using this methodology is that microfinance borrowing is associated with modest increases in the probability of some home improvements, particularly the replacement of adobe walls with concrete block walls and the replacement of dirt floors with concrete or tile floors. We also find some evidence that microfinance borrowing is positively associated with new roof construction, although large standard errors on these estimates preclude us from making strong conclusions in this area. We uncover little evidence of positive impacts from microfinance on the installation of indoor plumbing and land purchases.

The next section provides a brief review of the impact study literature, and how different impact methodologies have been applied to an analysis of microfinance. Section 3 considers our field research context, methodology, and econometric model. Section 4 presents our results, and Section 5 concludes with suggestions and caveats about the appropriateness of our approach to other contexts.

## 2. Impact Methodologies and Microfinance

Historically, researchers have used a number of methodologies to ascertain program impacts. Each offers advantages and disadvantages in terms of cost, accessibility of data, and unbiasedness of impact estimates. One traditional methodology, for example, has been a

before-and-after analysis of a treatment group relative to an ostensibly similar sample of individuals outside the treatment. An analysis of the "difference-in-differences" in this context is used to capture the difference in change among impact variables within the treatment group (see for example, van de Walle, 1999). While such studies are relatively straightforward to carry out, they require baseline surveys among both treated and untreated populations. Moreover, analyzing difference-in-differences without a randomly chosen treatment and control groups can lead to impact bias based on self-selection into the treatment based on unobservables, such as entrepreneurial drive, a predilection for self-improvement, or even raw IQ. Microfinance borrowers, in particular, are a self-selected group who are likely to possess characteristics that differ from the population norm. For example, entrepreneurial drive is likely to be much stronger among those seeking microfinance loans than a typical subject of a survey, and even a typical entrepreneur. As a result, problems with omitted variable bias are likely to cause an overestimation of treatment effects from microfinance.

While using basic difference-in-differences and panel survey data can provide quality estimations of program impact when carried out in the presence of a proper control group, the approach is often problematic to practitioners because it is both time-consuming and costly, especially if institutions have failed to undertake a baseline survey of treatment and control groups before program implementation. Beginning the study with a new baseline survey may delay obtaining valuable information about program effects far into the future.

Some have tried to skirt these problems by simply comparing old members of a treatment group with newer members, such as using newly enrolled borrowers in a microfinance program as a control group for old borrowers. This has been the approach undertaken in some development research, such as was undertaken by USAID in its AIMS research project. But as Karlan (2001) and Karlan and Alexander-Tedeschi (2006) point out, this kind of approach can lead to an "attrition bias" in which the performance of old

borrowers may exceed those of new borrowers because of a hidden qualities in old borrowers that have allowed them to remain in the program. Only a subset of new borrowers is likely to share these qualities, and hence the impacts observed by a researcher will be biased by this unobserved difference.

In other instances researchers have used instrumental variables to try to identify impacts. By using a third variable that is correlated with program access, but has no direct effect on the impact variables of interest, the use of instrumental variables can overcome problems of endogeneity to allow for theoretically unbiased estimates. Work of this kind often uses the fact that programs are often implemented in a staggered fashion, or utilize participation rules that can be exploited by researchers to analyze program impact.

Wydick (1999), for example, uses the staggered nature of the introduction of lending in different areas to help identify the degree of credit access granted to Guatemalan borrowers in estimating the effects of microfinance on child labor. In this approach credit effects on school enrollment are obtained using the staggered entry of a credit institution into different areas along with gross sales as instrumental variables for quantity of borrowing.

Pitt and Khandker's (1998) well-known study examines the impact of microfinance among a population of households who were located in areas served by the three largest microfinance institutions in Bangladesh, the Grameen Bank, RD-12, and BRAC. The authors exploit the program participation rules of the microlenders as an identifying instrument, which limit participation to poor households who owned less than 0.5 acres of land. Identification of impact from their study comes from looking at changes in consumption and other variables by borrowers marginally on either side of this participation rule. They find that consumption by households increased when loans were granted to women by about 18% of the amount borrowed.

The main difficulty with the use of instrumental variables is logistical; instruments, if they are available, differ from one situation to the next. Furthermore, finding instruments

such as Pitt and Khandker's that are strongly correlated with program access in a particular context, but uncorrelated with impact variables, also requires substantial ingenuity, complicating the use of a standardized instrumental variable approach. In the context of microfinance, finding convincing instrumental variables for credit access or actual borrowing has often proved to be a frustrating exercise for researchers (Armendáriz de Aghion and Morduch, 2005). What is more, instruments vary in their strength of correlation with program access; weak instruments yield imprecise estimates of true impact magnitudes. Exclusion restrictions of potential instruments from the main estimation are not always easily satisfied.

Matching models have been another way that people have sought to create artificial controls in order to identify treatment effects. This technique has been applied to microfinance by for example, Gómez and Santor (2003), who use a statistical matching model to identify the effect of group lending relative to individual lending among 1,389 individual and group borrowers in a Canadian lending institution. But matching models are often criticized in some contexts because they cannot control for unobservables such as entrepreneurial drive, which may govern self-selection into treatment and influence treatment impact, particularly in areas such as microfinance.

To overcome these problems, the use of randomized field experiments has become increasingly common in ascertaining the impacts of many types of poverty intervention programs (see Duflo, 2006). As yet there are few examples of randomized field experiments in microfinance.<sup>2</sup> But randomized field experiments have become popular because they allow for

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<sup>2</sup> While not explicitly a randomized experiment, Coleman's (1999) obtains a measure of microfinance impact in 14 villages in Thailand by using a quasi-experimental methodology in which borrowers who would receive microfinance loans in the future act as a control group for borrowers that were actually granted credit access. By including a dummy variable for credit participation by both those that seek credit in the control villages and those with access to credit in the treatment villages, he controls for self-selection issues. Coleman finds the impact of microfinance to be small, yet cautions that the impact may be diluted in his study based on the relatively high degree of wealth and widespread credit access of the borrowers throughout his sample population.

a maximum degree of exogeneity in treatment and control, allowing researchers to overcome the often-thorny issues of self-selection, endogeneity, and omitted variable bias.

Randomized field experiments, however, face their own set of challenges. To create the control group needed for the identification of treatment impact, it is necessary that some who desire access to the treatment (such as health, or education, or microfinance) remain untreated for a specified time so that impact can be measured on an equivalent treatment group relative to the control. This difference in the timing of treatment is usually justified by a constraint on the institution's ability to treat all agents immediately anyway, and in many cases a random lottery can actually be perceived as a fair way to determine the queuing rule. Yet it is essential that the control remain untreated by both the institution and competing institutions for the specified duration of the study.<sup>3</sup>

The challenge becomes that the longer desirous households are denied a treatment, such as access to microcredit, there is an increasing tendency for untreated units to “bleed” from the study by seeking the treatment (e.g. loans) from other sources. This may reduce the difference between the treatment and control groups in the experiment, and hence lead to downward bias in the estimated impact of the treatment. Thus because any synthetic research structure is difficult to maintain for a long period of time, randomized experimental studies rightly tend to be of short duration, and short-duration studies present several practical problems for the evaluation of interventions such as microfinance.

Any short-duration study may not yield the time period necessary for impacts to be realized from a treatment. It also can lead to dynamic self-selection problems (primary adopters of credit may exhibit different impacts than secondary adopters), and be subject to the influence of time-specific economic shocks that are complementary to a treatment, resulting in under-estimated standard errors. Finally, because short-term studies represent a snapshot of

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<sup>3</sup> “Encouragement designs” have offered one important way around the problem of withholding treatment.

program impact over a short time frame, they cannot capture important dynamics of treatment impact. Ideally, both practitioners and researchers would like to understand how *long* a given impact takes to become fully realized within a population and when the impact dies out.

Movies contain more information than photographs.

### 3. Methodology

The RETRAFECTION methodology seeks to address a number of these issues, while creating its own distinct set of challenges. We first carry out a household survey within a pool of microfinance borrowers that creates a retrospective panel of fundamental events regarding major dwelling changes. We combine this retrospective panel with historical variation in the timing at which different households had access to the treatment in order to estimate impacts. Related approaches have been carried out, for example, in ascertaining the impact of microfinance on fertility decisions (Morduch, 2004).

Our cross-sectional survey is of households from a set of villages in Guatemala who began participation in the microfinance program in at different times, beginning in 1993 until a year before our survey in 2005. Ideally one would like to obtain a random sample of program participants starting after a specified time in order to mitigate problems of "attrition bias," in which long-term participants may display different treatment effects than the average participant (see Karlan, 2001). However, as we show in this paper, even when only current program participants are surveyed, it is relatively straightforward to check, and even account for, attrition bias in the impact estimations. In our case, when we carry out straightforward tests for significant impact differences between old and new borrowers in the portfolio, even using different specifications, we find no systematic positive or negative difference in our estimations.

The cross-sectional survey used for the RETRAFECTION methodology is built around discerning the timing of memorable events in the history of a household. For example, a study

on the impact of a pre-natal health program on miscarriage and infant mortality could accurately collect recall data on miscarriages, births, and deaths of children, which are unforgettable events to any parent, but probably not on minor childhood illnesses. In our application to microfinance, we focus on the timing of upgrades in dwelling structure such as the upgrade of a home's walls from adobe to cement or a home's floor from dirt to cement or tile. However, historical questions on changes in most non-discrete events, such as revenues and profits in an informal sector enterprise, are inappropriate in the creation of a retrospective panel since their timing and precise quantities may be difficult for subjects to recall. Thus, unless careful historical records exist, a RETRAFECTION study can be used effectively only with discrete and psychologically significant dependent variables, or these "fundamental" events. The type of event chosen should be a likely direct or indirect effect of the treatment, rare and important enough to be memorable, but probable enough such that it can be meaningfully used as a dependent variable.

In our study, we took care to ascertain the timing of these major dwelling changes by referencing them off the ages of children and other key events in the life of the household and village.<sup>4</sup> There are advantages with impact studies using variables such as housing because qualities of a house are manifest to a surveyor visiting the household, and thus it is necessary only to help the household pinpoint the timing of when a dwelling improvement was undertaken. This contrasts with information on, say, infant mortality, for which the event itself must be elicited by the researcher, along with the timing. From our data we create a history for each household consisting of these discrete dwelling changes over time along with the timing of initial credit access and initial borrowing back to the time of occupation of the dwelling unit.

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<sup>4</sup> Using this methodology, it is helpful for the surveyor to create a chronological record of births of children in the family as well as deaths of parents and grandparents on a timeline during the survey, years which are likely to be fixed in people's minds. These events can then be used to pinpoint the timing of an event by asking whether certain children or grandparents were alive when, for example, a new house was built.

The sum of these recreated histories across households forms an (unbalanced) panel data set from which estimations are carried out.

Along with its ability to be implemented *ex-post* to program implementation, another practical advantage of the RETRAFECTION methodology is that it may allow for an estimation of treatment effects without the use of the standard control group. In a statistical sense, the differential timing of program participation allows households in the sample who access the program at different times to act as mutual controls. The idiosyncratic influences of the regional economy over different years are controlled for through year-level fixed-effects. The idiosyncratic differences between villages are dealt with through village-level fixed effects. The counterfactual in our methodology is identified within the sample itself by the probabilities of home improvements among the whole sample of borrowers before credit was introduced into each household's village. Thus one of the advantages of our methodology from a practitioner standpoint is the attraction of being able to form counterfactuals strictly within an institution's own client base. The fact that all in the sample (at least eventually) chose the credit treatment addresses problems of self-selection. We also utilize household controls such as mother's and father's education, age, initial wealth, and type of enterprise.

One drawback of the RETRAFECTION methodology is that when used with a treatment that is endogenously chosen by households, it estimates treatment effects that are different from the standard "intention to treat effect" (ITE) and "treatment effect on the treated" (TET) measures. Akin to the ITE, it estimates what we refer to as a *intention to treat effect on the eventually treated*, (ITEET) which is defined as the change in the probability of a fundamental event from a household having access to the treatment in its community, among those households who eventually take up the treatment. This is different from the standard ITE, which compares impact across a random sample of households within a community over a given time period from an available treatment when uptake is less than 100% within the community. Our second

measure we call a *take-up effect*, which is different from the standard TET in that it estimates treatment effects that include some households who choose the treatment well after it has been introduced into a community. This makes this measure more susceptible to possible demand-side endogeneity, and requires a series of diagnostic checks to minimize the likelihood that any positive relationship between treatment and impact is not influenced by omitted variables.

As a result, an important challenge with our approach is that data must meet certain specific criteria for exogeneity in order to ascribe causality in the relationship between treatment and impact. Hence, before we can begin to assess causality from the rollout of the program, it is necessary to test for supply-side and demand-side endogeneity identifiable in the data. In short, we attempt not to use words such as "impact" or "causality" lightly, and we outline the process of our econometric methodology in the following sequence of steps:

**Step 1: Check for supply-side endogeneity in the rollout of a program.**

The first set of diagnostics examines the pre-treatment outcomes across communities in order to see whether there are signs of endogeneity in the way in which the program was rolled out, or supply-side endogeneity. Direct questions to field directors regarding the nature of how programs have been rolled out across regions are important, but the independence of the rollout with respect to impact variables should be confirmed statistically. The first test calculates the mean pre-treatment outcome and regresses this on the year (or month) in which the program was first received. This diagnostic checks for whether economically good or bad communities were offered the program first. Given the use of fixed effects in the regression, however, this kind of endogeneity would not alone bias estimates of the ITEET. The second test checks for whether the pre-treatment *trend* in outcomes varies with the order of receipt of the treatment. If, for example, the program was offered first to those communities that were growing quickly anyway, then fixed-effects regression using retrospective panels will be

biased. We perform this test by taking the average first difference of the outcomes prior to treatment and regressing it on the year treatment was offered. If we find bias of this kind, we can proceed to run an ITEET regression which interacts a time trend with the village fixed effects, thereby allowing each community to have a different trend. If we do not find such bias, then we can proceed using a standard two-way fixed effects regression. The third test uses only pre-treatment data and runs a two-way fixed effects regression including a dummy for the year prior to the receipt of the treatment. If this term is found to be significant, then it indicates that the treatment may have been systematically introduced in response to some kind of shock, and any mean-reversion in outcomes will likely lead to a biased estimate of the ITEET. If the rollout passes this battery of endogeneity tests, then we can proceed to its estimation.

### **Step 2: Estimation of the Intention to Treat Effect on the Eventually Treated (ITEET)**

In the absence of evidence pointing to supply-side endogeneity in the rollout of the program, we can proceed to Step 2, in which we carry out an estimation of the ITEET. The ITEET reveals the impact from the existence of an exogenously implemented program in the village among those who eventually take the treatment. Because these data do not indicate the presence of supply-side endogeneity, we can estimate the ITEET by regressing the impact variables on a dummy variable indicating the presence of the program beginning in the year when it was introduced in each geographical area.

### **Step 3: Testing for Demand-Side Endogeneity**

If we find a positive effect from the ITEET, in Step 3 we carry out a diagnostic test for demand-side endogeneity by including a “no program” dummy variable that indicates the absence of program access. This dummy variable is interacted with the pre-treatment time period dummy variables in the regression we describe in more detail in the

next section. If this interaction term is either positively or negatively significant, it means that there are differences in the pre-treatment impact variables between those in the sample with access to the treatment and those without, indicating the existence of an Ashenfelter's Dip (see Ashenfelter, 1978). The sign of this interaction term will indicate, for example, if people take microloans when they are facing positive shocks (perhaps reflecting added economic opportunity) or negative shocks (which would indicate consumption smoothing).

An additional diagnostic we employ to check for demand-side endogeneity is to restrict the estimation sample to those who received credit shortly after the program entered the village. If these estimates are significantly different from the unconstrained sample estimates, then it may indicate demand-side endogeneity because it implies that omitted variables may be causing households to take credit that are correlated with credit take-up and impact variables. If these estimates are statistically similar, then demand-side endogeneity is likely to be less of a concern since those freshly exposed to credit exhibit similar effects to those "choosing" credit later. Still, results must be treated with some caution. In general, with any treatment in which take-up is optional for households, it is possible that the treatment has been taken in order to specifically achieve the outcome which we measure, and so we cannot interpret a test for differences in outcomes after individuals choose a treatment as necessarily causal.

#### **Step 4: Estimation of the Take-up Effect**

If we find no evidence of demand-side endogeneity in Step 3, then we can regress the impact variables on a simple dummy variable equal to one in years when the treatment was adopted by a household to estimate the take-up effect. The take-up effect estimates the average change in the probability of the fundamental event during the years after credit has been taken relative to years before credit was taken. Some researchers may desire to interact

the treatment dummy variable with household characteristics to ascertain among which household types the treatment displays the strongest take-up effects.

### **Step 5: Treatment Window Regression and F-test of Take-up Effects**

There is a trade-off in determining the width of the treatment window that includes the number of pre-treatment and post-treatment years used in the final take-up effect estimation. A larger window reveals longer-term effects, but drops observations in which households have obtained the treatment relatively recently. A smaller window includes these observations, but reveals less about the dynamics of impact. We believe it reasonable that most housing effects would occur within a 2-3 year time period after credit access (and our data appear to show a tapering off after this point), but other types of effects may suggest a shorter or longer window. The treatment window also yields the estimated parameters necessary for a test of take-up effects via an  $F$ -test. Similar to “event studies” in the finance literature, here one tests for differences between changes in probabilities in home improvements in post-treatment years versus pre-treatment years within the window. If Step 3 reveals the presence of demand-side endogeneity from significant pre-credit interaction terms, the  $F$ -test for program impact may still be carried out. However, the counterfactual then used in the test (by inclusion of the interaction term) becomes observations of households without access to the treatment rather than simply households who have not taken the treatment.

## **3. Field Research and Estimation**

While most microfinance loans, including those in our survey, are intended for business investment and not housing, there is reason to believe that increased profits from microloans should indirectly result in housing improvements.<sup>5</sup> In Guatemala as in many other

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<sup>5</sup> It is also possible that microloans intended for enterprise capitalization may be diverted into use for dwelling improvements. An anonymous Bolivian MFI estimates that 20 percent of its “microenterprise” loans go for

contexts, housing differs from other goods in that it not only represents an important consumption good, but also a major store of wealth and a measure of prestige. Tax (1953), for example, observes that social status among rural Mayans in Guatemala is often reflected in the quality and size of homes and land. For this reason, improvements in houses and land are typically among the first changes rural households make when family income begins to increase. In rural Guatemala this is particularly important, because in rural areas homes are infrequently bought and sold, but rather are inherited by offspring who continue to reside on the same plot of land.

The context for our field survey was rural western Guatemala in several villages surrounding the cities of Quetzaltenango and Mazaltenango. In Guatemala, the majority of the population lives in rural areas, high even by Latin American standards. Virtually all of those in our survey were Mayan Indian households living in subsistence agriculture on plots of land in which the household grows corn, beans, coffee, and sometimes plots of vegetables. Only 28% of the borrowers in our survey attended secondary school. Average age is approximately 39, about 35 for men and 41 for women; in our sample 65% are female borrowers. Almost exactly 50% of borrowers in the survey identified themselves as evangelicals, while the other 50% identified themselves as Catholic, typical for the area.

Our empirical estimations are taken from data collected during the summer of 2005 in a survey of 218 rural households located in 14 different villages. The sample selection was coordinated with the help of *Fe y Alegria* (trans. *Faith and Joy*), a medium-sized Jesuit-run microfinance institution in Guatemala operating since 1993, that grants microloans to around

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home construction and expansion (Center for Urban Development Studies, Harvard University Graduate School of Design, 2000). Nevertheless, some research has pointed out that investing in dwellings may not necessarily represent a complete diversion of credit, since such improvements may increase the income-generating potential of home-based activities (Ibid.). As a response to this phenomenon, many MFIs have become interested in developing new lines of micro-credit specifically to finance housing (Ferguson, 2004). In Guatemala for example, Génesis Empresarial, a Guatemala City-based MFI, has a small portfolio of borrowers with home improvement loan products that carry average terms of two years.

3000 clients per year. For the purpose of this study, borrowers were selected from two major regions serviced by *Fe y Alegria*, the predominantly rural regions around the city of Quetzaltenango and in and around Mazatenango. Quetzaltenango is part of the western highlands, with villages ranging between 7000 and 8500 feet above sea level, where nights are cold and daytime temperatures rarely exceed 85 degrees Fahrenheit. Mazatenango lies near the coast with a warmer and more humid climate. The sample was taken from a list of current borrowers of *Fe y Alegria* in both regions. All borrowers were engaged in light manufacturing activity, including tailoring, candle-making, and carpentry, while others were commercial vendors, small retailers, and livestock owners.

The questionnaire was intended to measure changes in our different categories of dwelling improvement: upgrades to walls, roofs, floors, plumbing, and increases in land. Each borrower was asked about changes in these variables during the history of the household, and the timing of these changes. For example, we asked households how long they had lived in that specified location. If a household had cement walls, we asked them if a different kind of wall structure existed since they had lived in that location. If prior to the cement walls, the house had had adobe walls, we asked what year the upgrade had taken place. Clearly, a reasonable concern with this kind of survey method is the problem of inaccuracy in the creation of retrospective panel data set. Because, for example, the upgrade of floors from dirt to cement poses such a major augmentation in quality of living standards for a family, and because the most recent potential changes are manifest to the surveyor, there was relatively little problem with the recall of such events. We believe that errors in ascertaining the year of the dwelling upgrade were minimized through referencing the event off a chronology of family births and deaths.

From the survey we then create an unbalanced panel data set. The unbalanced nature of the panel data arises because the model considers the number of years the head of household or

borrower had lived in the present site as the defining number of years used in the time series for each household. Our estimations were carried out on data beginning in 1990, but some households had resided in a particular locale only after 1990.

### *Estimation Technique*

Our model first estimates the probability of one of our households upgrading from a low quality material to a high quality material in the structure of the house. For walls this is from either adobe to finished adobe, or adobe (finished or not) or wood to cement. For roofs this is from either palm leaves or corrugated iron to either cement or tile. For floor upgrades, the changes we analyze are from dirt to cement, cement to tile, or dirt directly to tile. With changes in toilet, our upgrade is from an outhouse to indoor plumbing. Lastly, we consider land purchases, important to rural Guatemalans because land in this densely populated region is used as a store and measure of wealth and a sign of rural prosperity.

Probit and logit models are most commonly used for estimations in cross-sectional qualitative estimations and sometimes in panel data. However, we favor the linear probability model, which has become increasingly used in panel data estimations, since as a linear estimator it produces more robust estimates when implemented with fixed-effects estimations, especially with “wide” panels (Chamberlain, 1980). Estimations are conditional, of course, upon a household not previously having made the particular type of dwelling upgrade.

The two-way fixed-effects model we estimate is the following:

$$y_{it} = v_j + \alpha_t + \sum_{n=1}^N \beta_n X_n + \sum_{t-\bar{t}=-k}^k \tau_{i,t-\bar{t}} T_{i,t-\bar{t}} + u_{it}, \quad (1)$$

where  $y_{it}$  is a bivariate dependent variable that is equal to 1 if household  $i$  upgrades walls in year  $t$ . (And similarly for separate estimations on roof, floor, toilet, and land.) For the independent variables,  $v_j$  is a village-level fixed effect,  $\alpha_t$  is a year-level fixed effect, the  $X_n$  are a set of  $N$

household controls such as education, age, age squared, enterprise type, and initial wealth;  $u_{it}$  is a mean zero error term. The fourth term is the estimation on a sequence of treatment dummy variables,  $T_{i,t-\bar{t}}$ , that comprises a “treatment window” of length  $w$  years representing a sequence of lags and leads surrounding year  $\bar{t}$  for household  $i$ . The treatment dummy variable is equal to 1 if household  $i$  first received a microfinance loan  $t-\bar{t}$  periods “ago,” and zero otherwise. If  $t-\bar{t}$  is negative, it means that household  $i$  received credit  $t-\bar{t}$  years *forward* from time  $t$ . For a symmetric treatment window of width  $w$  around the time of treatment, then the summation in the fourth term of the model includes  $k = (w-1)/2$  years of leading treatment dummies,  $k = (w-1)/2$  years of lagged treatment dummies, as well as the contemporaneous dummy for when  $t = \bar{t}$ , for the year in which the household first received microfinance. For example, consider a treatment window of  $w = 5$  for a household  $i$  that initially received microfinance in 2001. For the observation of household  $i$  in the year 2000, the data in the retrospective panel then contains a vector of treatment dummy variables--0, 1, 0, 0, 0--which correspond to estimated coefficients  $\tau_{i,-2}, \tau_{i,-1}, \tau_{i,0}, \tau_{i,+1}, \tau_{i,+2}$ . For the observation of household  $i$  in the year 2003, the vector of dummy variables would be 0, 0, 0, 0, 1.

Many upgrades to homes took place during the surveyed history of our households, our key set of dependent variables. At the time the current borrower began residing in the household, 109 of our houses had been constructed with the inferior wood or adobe walls (86 adobe, 23 wood). During the history of the household, 61 of these houses had upgraded to cement block. Similarly, 193 of the houses initially had either dirt or cement floors (97 dirt, 96 cement). During the history of the household, 68 had upgraded, either from dirt to cement, dirt to tile, or cement to tile. With respect to roofs, 139 roofs were initially of corrugated iron or palm leaves (137 corrugated iron, 2 palm leaves), and 25 had been upgraded to either cement or tile. In our survey, 133 of our households initially had access to only an outhouse,

and 52 of these households installed indoor plumbing at some point in the current household's history. In land purchases, 49 households had realized changes in landholdings, with 44 acquiring more land and 5 selling land.

We present basic tabulations from our household survey in Table 1A as shown by lagged values at one year before microfinance borrowing. Along with presenting a picture of dwelling characteristics, this represents a crude look at very short-term changes around initial borrowing. The figure in parenthesis in the “Pre-Credit” columns excludes exclude borrowers receiving credit in 2005 who do not appear in final columns for ease of comparison.

The first part of Table 1A shows changes in wall structure from approximately one year before and one year after credit. Before credit, houses with block walls constitute 51.9% the sample. Houses with (inferior) wood (7.0%), adobe (30.8%) and walls made of adobe finished with lime whitewash (10.3%) round out the sample. The changes appear to be uniformly positive in the window around initial microfinance borrowing: the percentage of houses with concrete block walls increases from 96 to 113 (51.9% to 61.1%) while the number of houses with adobe walls decline from 57 to 45 (30.8% to 24.3%). Wood-wall houses also decline from 13 to 9 (7.0% to 4.8%). However, since we are not yet controlling for time via year-level fixed-effects, it is impossible to tell if these changes are the result of a general time trend or if they are influenced by the credit treatment.

We see a similar story with changes in roofs. Concrete roofs increase from 22 to 31 (11.9% to 16.8%) while corrugated iron roofs decrease in the sample from 115 to 109 (62.2% to 58.9%). Clearly there is some movement from both corrugated iron and tile roofs to concrete, but again without accounting for year fixed-effects it is impossible to attribute such changes to credit.

Table 1A also shows similar patterns with changes in floors and toilets. One year after credit, both tile and concrete floors increase a few percent in the sample compared to one year

before credit, while dirt floors decrease commensurately. Houses indoor plumbing increases from 87 to 99 pre- to post-credit in the sample, while houses only having an outhouse decline from 92 to 82, a shift in about 5% of the households, a seemingly high rate of change within only (approximately) two years.

Table 1B gives base probabilities of these dwelling upgrades, conditional upon the upgrade not having already occurred in the history of the household (the criterion we use in our estimations). The probabilities of upgrades in any given year for households across the sample range from 0.017 (new roof), 0.030 (new floor), 0.0375 (new toilet), 0.040 (land purchase) 0.048 (new walls), to 0.148 (for any of these). Means and standard deviations of control variables are also given in Table 1B.

#### 4. Impact Estimation Results

We will work through a summary of our results based on the steps that outline our methodology. Our diagnostic checks in Step 1 for supply-side endogeneity are presented in Table 2. Regressing average pre-treatment outcomes (new walls, new roof, new floor, new toilet, new land) on the year in which credit was offered to the village, we find no evidence of supply-side endogeneity in the levels of the pre-treatment outcome based on the year that the credit program was introduced into villages. We also find no evidence of supply-side endogeneity in a pre-treatment trend of more rapid or less rapid changes in home improvements (our second estimation in Table 2). Our last diagnostic is a check for the entrance of the program as a result of previous-period shocks to households. Again, we find no evidence that the provision of credit to a village is a function of an abnormally greater or lower rate of home improvement in a village the year before credit program entry. As a result, we uncover no evidence that entry of the credit program in different areas is endogenous to our impact variables.

We proceed with Step 2, our estimation of the ITEET. Here we estimate the increase in the probability of the different home improvements simply as a function of the existence of the credit program in a household's village. In Table 3A we find modest evidence that the existence of the program in a village is associated with higher propensity for home improvements among eventual borrowers. We estimate an increase in the per year probability of a wall upgrade of 3.9 percentage points, significant at the 95% confidence level. We also find a point estimate in the increase in the probability of land purchases of 3.1 percentage points and of any one of our five home improvements of 6.0 percent points, however, *t*-statistics on the latter are marginal, only 1.30 and 1.53 respectively. For roofs, floors, and toilets we find point estimates close to zero or slightly negative and all statistically insignificant. Table 3B includes individual household characteristics and terms in which the treatment is interacted with these characteristics. We find little significance here other than that the effect of credit on new wall construction appears to be realized heavily among rural households with livestock operations, and the impact of credit access among households on the installation of new indoor toilets is proportionally greater.

Our results for Step 3, the test for demand-side endogeneity, are given in Table 4. First, we create a dummy variable indicating whether or not the credit program had been introduced into the village of household *i* at time *t*. We interact this dummy variable with the variables representing the years within the treatment window for each household prior to treatment and include it in the estimation along with the other pre-treatment time dummies. We then estimate the equation

$$y_{it} = v_j + \alpha_t + \sum_{n=1}^N \beta_n X_n + \sum_{\bar{t}=t-k}^k \tau_{i,t-\bar{t}} T_{i,t-\bar{t}} + \sum_{\bar{t}=t-k}^{-1} \delta_{i,t-\bar{t}} T_{i,t-\bar{t}} d_{i,t-\bar{t}} + u_{it}, \quad (2)$$

where there are  $w = 2k + 1$  leading and lagged treatment dummy variables in the second summation and  $k$  leading treatment dummy variables in the third summation interacted with a dummy equal to 1 if the microfinance program was *unavailable* and zero otherwise, where  $d_{i,t-\bar{t}}$  is the interacted dummy representing the absence of a credit program. Identification comes in a five-year treatment window from the 42 households who accessed credit within one year after the introduction of the program.<sup>6</sup>

Significance of the  $\delta_{i,t-\bar{t}}$  coefficients in (2) could reflect endogenous borrowing in the following ways. On one hand it is conceivable that microenterprise entrepreneurs might choose to borrow in good economic times, in order to take advantage of economic opportunity. Good economic times could thus initiate borrowing, but also cause high profits by themselves and thus cause dwelling upgrades. Failing to correct for lack of program access would thus *overestimate* the difference between post-credit and pre-credit treatment variables, *i.e.* demand-side endogeneity would bias the difference between post-credit outcomes and pre-credit outcomes *upwards*. This would bias our  $F$ -test on impact toward a propensity for Type I errors (rejecting a null hypothesis that there is no significant change in probability of dwelling upgrades yielded after credit). In the presence of endogenous borrowing based on positive economic opportunity, we would thus expect the  $\delta_{i,t-\bar{t}}$ 's to be positive. The true change in probability of dwelling upgrades for a pre-credit year would not be  $\tau_{i,t-\bar{t}}$ , but rather  $\tau_{i,t-\bar{t}} + \delta_{i,t-\bar{t}}$ .

Another source of endogeneity between borrowing and dwelling upgrades could be from an opposite phenomenon: Microenterprise entrepreneurs might choose to systematically borrow when prices happen to be low for their particular product (or economic times are hard) in order to smooth negative shocks. Here, failing to correct for lack of pre-credit program access would

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<sup>6</sup> In our sample, 28 borrowers obtained credit in the first year a the program was introduced into a village (allowing of an observation on the probability of a dwelling upgrade one year before credit when there was no credit access as well as two years before), and 14 obtained credit one year after (allowing for an observation when there was no credit access on two years before).

*underestimate* the difference between post-credit and pre-credit treatment variables, making our  $F$ -test biased *downwards* and inclined toward Type II errors, accepting the null of no significant change in probability of dwelling upgrades yielded after credit. With this type of endogeneity, we would expect the  $\delta_{i,t-\bar{t}}$ 's to be *negative* since with the unavailability of credit, negative shocks would further reduce the probability of dwelling upgrades.

To test for this kind of systematic demand-side endogeneity in borrowing decisions, we carry out a test for the significance of the  $\delta_{i,t-\bar{t}}$ 's on whether they are jointly different than zero. If these interactive dummies are jointly *significant* by an  $F$ -test, meaning that significant demand-side endogeneity exists, then our new test for the effect of microfinance borrowing on dwelling upgrades would then become the significance of differences between the post-credit treatment  $\tau_{i,t-\bar{t}}$ 's and the sum of the pre-credit  $\tau_{i,t-\bar{t}} + \delta_{i,t-\bar{t}}$ 's within the symmetric treatment window. If the interacted variables are jointly *insignificant*, then the decisions of previously credit-constrained households and previously non-credit constrained households are insignificantly different, and we can use our standard  $F$ -test differences between the sum of the post-credit  $\tau_{i,t-\bar{t}}$ 's and the sum of the pre-credit  $\tau_{i,t-\bar{t}}$ 's within the symmetric treatment window.

As seen in Table 4, we find little statistical evidence for the joint significance of these interacted dummy variables. In none of our five dwelling changes or our general home improvement variable are the interacted variables on the raw pre-treatment dummy variables and the microfinance constraint dummy (NOPROGCREDMINUS1 and NOPROGCREDMINUS2) jointly significantly different from zero at even the 10% level. (Even the greatest significance, on NEWROOF is only at the 25% level.) We must qualify the power of these tests, since with a treatment window of 5 years, they rely on this subset of 42 households who took credit either the year that the program was introduced in a village or one year after. Nevertheless, even a sub-

sample of this size is likely to pick up significant endogeneity between the timing of credit choice and the timing of dwelling upgrades.

In line with our Step 4 we carry out a simple estimation of the take-up effect of microfinance borrowing. Table 5A shows the estimation on a dummy variable reflecting a household's participation in the microfinance program. The coefficient is significant at the 95 percent level for NEWWALLS and NEWFLOOR, positive but insignificant for NEWROOF, and zero or negative for our other impact variables. Adding the household control variables and interactive terms yields little additional insight other than that the impact of credit on rural home improvements again seems to be higher among rural livestock owners, and so for space considerations we don't report these results here.

In Table 5B we carry out the same estimation as in 5A, but as an additional diagnostic check for demand-side endogeneity, we restrict our sample to those households who took credit shortly after the program was introduced into the village. Here we include restrictions on our sample for those who took credit within 2 years or less of program entry into the village, 1 year or less, and those who took it immediately in the first year of credit availability. Again, we find consistently positive (and even somewhat larger) point estimates on new walls, mostly positive estimates on new floors, larger and significant estimations on all home improvements, and fairly large point estimates on new toilet, which were insignificant in Table 5A. Because sample sizes are reduced from the restriction, standard errors also increase, but the fact that point estimates are consistently in the range of our estimations in 5A offers additional evidence that the significance of our take-up effects are unlikely to be generated by demand-side endogeneities. Still it is important to exercise caution in the interpretation of coefficients in this type of treatment, an issue we will discuss more fully in the conclusion.

We also need to consider that our impact estimations could be affected by an attrition bias based on the composition of an institution's borrowing portfolio. The phenomenon here could be

that old borrowers could represent a group that exhibits different responses to credit than newer borrowers, since some borrowers (for whom the impact of loans could be greater or smaller) may have dropped out of the pool from an old cohort. Our methodology avoids the kind of attrition bias, for example as described in Karlan (2001), in that identification our methodology relies on the specific *timing* of dwelling changes after microfinance borrowing rather than the simple differences in impact variables between old and new borrowers. Thus, ideally for this type of study one would like to have a random sample of borrowers that includes former borrowers so that estimations are carried out on all recipients of credit after a given year. Although we don't report the estimations here to respect space considerations, inclusion of a "old borrower" dummy variable and different specifications in which this variable was interacted with treatment status revealed no systematic differences in our impact estimates between old and new borrowers.

Table 6 shows our estimations using a five-year treatment window. In Table 6, 11 out of 12 of our post-credit coefficients are positive. The *F*-test of the hypothesis that the two post-treatment coefficients are jointly equal to the two pre-treatment coefficients is rejected at the 95% level for NEWWALLS and NEWFLOOR, but is insignificant in other impact variables due to high standard errors. Most point estimates of impact suggest an increase in the probability of the different home improvements on the order of 0.03 to 0.08 percentage points per year two years after taking credit. This magnitude appears small, but base probabilities of any of our home improvements in a given year range in the neighborhood of 0.02 to 0.05. Thus the fact that we find point estimates in the 0.03 to 0.08 range for most of our home improvement impact variables in the two years after credit suggests that microloans may well be having significant welfare effects, such that the probability of some home improvements appears to more than double in the two years subsequent to credit (over a small base probability). The estimate for new (concrete) walls from adobe walls suggests an even somewhat larger effect. Nevertheless, many of our point estimates have large standard errors, especially in our post-treatment

coefficients, rendering positive point estimates on NEWROOF, for example, to be insignificant. We also carried out estimations on larger treatment windows, but find that with the larger windows standard errors increase significantly in our data with the large number of dropped observations, causing us to feel that a five-year window most appropriate in our context.

Illustrations of the changes in the probability of all of our home improvement impact variables from the five-year treatment window estimated in Table 6 are given in Figures 1 through 6. The figures show that for many of the impact variables, particularly NEWWALLS, NEWROOF, and NEWFLOOR, probabilities of these home-improvement rise after accessing the treatment, but the 90% confidence level band in the figure also widens.

## 5. Summary and Implications for Future Research

The methodology we present in this paper involves the creation of a retrospective panel database taken from a single survey. This retrospective panel re-creates a history of major changes in the household over time, the timing of these major changes then being analyzed with respect to the timing of a treatment. Based on the timing of these major events within a treatment window, it becomes possible to analyze the subsequent changes in the probabilities of important variables correlated with economic development.

We think it may be helpful at this point to note where a RETRAFECTION study is most likely to be useful, and highlight important caveats with its implementation. First, to be able to fully attribute relative changes in post-treatment coefficients to program impact, a researcher must try to identify a program of interest that, at least anecdotally, has been phased in over time in a manner that is unrelated to impact variables (changes in dwelling units, health, capitalization of an enterprise, etc). When there statistical tests confirm that implementation of the program has been exogenous to household impact variables, the potential remains for interpreting impact estimates more confidently in terms of causality.

When using retrospective survey questions, use of this methodology should focus on correctly ascertaining the timing of fundamental events. Changes in variables such as profit, revenue, and so forth are difficult for subjects to remember, and are often imprecise by their very nature in informal-sector enterprises, often even when trying to be ascertained in the present. Major diseases, deaths, school enrollments, and major asset purchases are the kinds of variables best used within this framework. In many respects, this may not represent a disadvantage since what researchers (and households in development countries as well) often view as “development” may be closely associated with these kinds of fundamental changes.

It is worth considering when the “take-up effect,” defined by impacts that occur after households *choose* to take a treatment, can be interpreted causally. In our context, microfinance is a treatment that is always a household choice representing a means to an end, an end that may include improving living conditions via higher enterprise profits. Microfinance does not *cause* dwelling improvements *per se*, but may represent a door, perhaps a necessary door in some cases, that a household can pass through to best realize welfare improvements through releasing credit or liquidity constraints. But what truly *causes* these improvements is a particular household's *desire* for them, which in sequence may “cause” the household to take a microfinance loan and subsequently utilize enterprise profits for dwelling improvements. Thus when using the RETRAFECT methodology on treatments that involve household choice, point estimates may also be influenced by unobservable factors that led the household to *take up the treatment at a particular time*, making it unwise to infer a strictly causal relationship even when controlling for observable forms of demand-side endogeneity. However, in contexts where the methodology is implemented on treatments where adoption is nearly instantaneous with access and adoption is universal, point estimates on impact variables can be more appropriately interpreted as causal when supply-side diagnostic tests for endogeneity are insignificant.

Examples of other applications of community-wide treatments would include a randomly assigned vaccination program in public schools which is either mandatory or for which the benefits are so clearly manifest that everybody immediately chooses treatment. A second example might be the phase-in of clean water systems in a number of villages over time; everyone prefers the clean water to what existed before. A third example would be a road that, as it extends, connects villages one-by-one to urban areas. But even when applied to contexts in which households exhibit choice over treatment uptake, estimations yield valuable information that constitute strong associated welfare changes with a treatment, and we present the methodology using this type of treatment in part to illustrate how the more difficult cases may be handled. In the examples where uptake of the treatment is instantaneous and universal, Steps 4 and 5 are unnecessary and estimates can be interpreted as the standard Treatment Effect on the Treated (TET).

We view the principal advantages of the RETRAFECT methodology as being its ability to be employed using entirely *ex-post* data from a single survey wave, utilizing the sequencing of a project or program's rollout as a natural experiment. Moreover, it can be implemented within an institution's own client base, and unlike many other approaches it can explicitly trace out the dynamics of impact. We outline the circumstances and provide examples from our own data under which statistical significance can be attributed to standard causality, and suggest a sequence of diagnostic checks and possible remedies when different types of endogeneity are present in the data. The results from our particular application of the technique, that access to credit is associated with moderate increases in some variables associated with household welfare, appear to match closely with anecdotal evidence from the field and previous microfinance impact studies.

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**Table 1A: Frequencies of Dwelling Type  
(Pre- and Post- Credit)**

<b>Walls</b>	<b>--- Pre-Credit (<math>\bar{t}-1</math>) ---</b>		<b>--- Post-Credit (<math>\bar{t}+1</math>) ---</b>	
	<b>obs.</b>	<b>percent</b>	<b>obs.</b>	<b>percent</b>
block	106 (96)*	52.5 (51.9)	113	61.1
finished adobe	22 (19)	10.9 (10.3)	18	9.7
adobe	61 (57)	30.2 (30.8)	45	24.3
wood	13 (13)	6.4 (7.0)	9	4.8
total**	202 (185)	100.0 (100.0)	185	100.0
<b>Roof</b>	<b>--- Pre-Credit (<math>\bar{t}-1</math>) ---</b>		<b>--- Post-Credit (<math>\bar{t}+1</math>) ---</b>	
concrete	27 (22)	13.4 (11.9)	31	16.8
tile	51 (46)	25.2 (24.9)	44	23.8
corrugated iron	122 (115)	60.4 (62.2)	109	58.9
palm leaves	1 (1)	0.5 (0.5)	1	0.5
total	202 (185)	100.0 (100.0)	185	100.0
<b>Floor</b>	<b>--- Pre-Credit (<math>\bar{t}-1</math>) ---</b>		<b>--- Post-Credit (<math>\bar{t}+1</math>) ---</b>	
tile	25 (23)	12.4 (12.4)	26	14.05
concrete	118 (108)	58.3 (58.4)	114	61.6
dirt	58 (53)	28.7 (28.6)	45	24.32
total*	202 (185)	100.0 (100.0)	185	100
<b>Toilet</b>	<b>--- Pre-Credit (<math>\bar{t}-1</math>) ---</b>		<b>--- Post-Credit (<math>\bar{t}+1</math>) ---</b>	
	<b>obs.</b>		<b>obs.</b>	<b>percent</b>
indoor plumbing	97 (87)	48.0 (47.0)	99	53.51
outhouse	99 (92)	49.0 (49.7)	82	44.3
total	202	100.0	185	100.0
<b>Land</b>	<b>--- Pre-Credit (<math>\bar{t}-1</math>) ---</b>		<b>--- Post-Credit (<math>\bar{t}+1</math>) ---</b>	
		<b>mean &amp; std. dev</b>		<b>mean &amp; std. dev</b>
mean: cuerdas***	195 (178)	2.962 (2.940)	178	3.041
standard deviation		3.85 (3.73)		3.72
* values in parenthesis exclude borrowers receiving credit in 2005 who do not appear in final columns				
** totals may not equal category sum due to unrecorded observations for individual categories				
*** equals approximately 25 x 25 meters				

**Table 1B: Summary Statistics of Variables**

Variable	Mean	Std. Deviation	Max	Min
<b>Dependent Vars.:</b>				
New Walls*	0.0480	0.213	1	0
New Roof*	0.0170	0.129	1	0
New Floor*	0.0303	0.171	1	0
New Toilet*	0.0375	0.190	1	0
New Land	0.0402	0.5948	1	0
Home Improvmnt.	0.1481	0.3552		
<b>Control Variables:</b>				
Educ. Men (Years)	4.06	3.61	15	0
Educ. Wm (Years)	2.41	3.12	18	0
Age--Male	35.03	9.60	75	14
Age--Female	31.01	8.86	63	19
Initial Land	2.60 cuerdas	3.62	20	0
Retail	0.751			
Livestock	0.90			
Manufacturing	0.406			
* Probability of new improvement conditional upon not already having realized the improvement.		Dates of Credit Introduction into Villages (no. of households/village): V1: 2001 (20); V2: 2001 (3); V3: 1998 (47); V4: 1998 (10); V5: 2000 (40); V6: 2000 (4); V7: 2004 (3); V8: 2001 (3); V9: 2000 (6); V10: 1999(14); V11: 1998 (8); V12: 1999 (9); V13: 1995 (2); V14: 1993 (31).		

**Table 2: Tests for Supply-Side Endogeneity**

<b>1A. Is there endogeneity in the levels of the pre-treatment outcome? (Regress average pre-treatment outcome on the year in which credit was offered to the village.)</b>					
	New Walls	New Floor	New Roof	New Toilet	New Land
Year of rollout	-0.0018	0.0025	0.0002	0.0013	0.0111
	(0.003)	(0.004)	(0.001)	(0.001)	(0.010)
Observations	14	14	14	14	14
R-Squared	0	0.04	0	0.21	0.04
<b>1B. Is there endogeneity in the pre-treatment trend? (Regress average of the 1st difference of the pre-treatment outcome on year credit offered.)</b>					
	New Walls	New Floor	New Roof	New Toilet	New Land
Year of rollout	0.0006	0.0011	-0.0001	0.0001	0.0110
	(0.001)	(0.002)	(0.000)	(0.000)	(0.012)
Observations	14	14	14	14	14
R-Squared	0.02	0.06	0	0.08	0
<b>1C. Is the rollout endogenous to shocks? (Run FE regression using only pre-treatment data w/ dummy for 1st lead of year credit offered.)</b>					
	New Walls	New Floor	New Roof	New Toilet	New Land
ITE lead 1	0.0253	-0.0066	0.0014	-0.0087	0.0119
	(0.018)	(0.014)	(0.009)	(0.023)	(0.029)
Observations	887	1215	1191	956	1318
R-Squared	0.01	0.02	0.01	0.06	0.04
Robust standard errors in parentheses.					

**Table 3A—Intention to Treat Effect on the Eventually Treated (ITEET)**

	(1)	(2)	(3)	(4)	(5)	(6)
	newwalls	newroof	newfloor	newtoilet	newlandd	homeimprv
Credit Prog. Available	0.039**	0.003	-0.012	0.004	0.031	0.060
	(0.019)	(0.012)	(0.014)	(0.017)	(0.024)	(0.039)
Constant	-0.005	0.023	0.029	0.031	0.028	0.028
	(0.032)	(0.015)	(0.017)	(0.019)	(0.030)	(0.050)
Observations	1159	1359	1991	1298	2359	2359
R-squared	0.04	0.01	0.01	0.03	0.03	0.04

**Table 3B— ITEET with Individual Characteristics**

	(1)	(2)	(3)	(4)	(5)	(6)
	newwalls	newroof	newfloor	newtoilet	newlandd	homeimpr
Credit Prog. Available	0.201	0.055	0.054	-0.077	0.043	0.161
	(0.202)	(0.106)	(0.115)	(0.143)	(0.116)	(0.102)
education father	0.001	-0.000	0.003	-0.000	0.004	0.004
	(0.002)	(0.002)	(0.003)	(0.002)	(0.004)	(0.005)
education mother	-0.002	0.003*	-0.002	-0.001	0.003	0.000
	(0.003)	(0.002)	(0.003)	(0.002)	(0.005)	(0.004)
age of father	-0.003	0.004	0.001	0.002	-0.026***	-0.024***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)
age father squared	4.0e-05	-3.8e-05	-5.3e-06	-6.5e-05	2.8e-04***	2.5e-04***
	(5.9e-05)	(7.6e-5)	(6.1e-05)	(7.7e-05)	(7.8e-05)	(8.6e-05)
initial land (cuerdas)	-0.002	-0.000	-0.001	-0.000	-0.002	-0.002
	(0.002)	(0.003)	(0.002)	(0.002)	(0.005)	(0.006)
retail	-0.006	-0.002	-0.013	-0.034	0.049*	0.022
	(0.022)	(0.008)	(0.022)	(0.027)	(0.025)	(0.037)
livestock	0.019	0.026	0.005	-0.024	-0.006	-0.009
	(0.026)	(0.024)	(0.029)	(0.025)	(0.029)	(0.040)
educ father*program	-0.009*	0.001	-0.003	0.005	-0.008*	-0.013**
	(0.005)	(0.004)	(0.002)	(0.003)	(0.004)	(0.005)
educ mother*program	0.008	-0.001	0.001	0.013*	0.001	0.004
	(0.007)	(0.004)	(0.003)	(0.006)	(0.005)	(0.004)
age father*program	-0.007	-0.003	-0.001	-0.002	0.005	0.001
	(0.011)	(0.005)	(0.006)	(0.008)	(0.006)	(0.005)
age father^2*program	6.9e-05	3.1e-05	3.8e-05	8.4e-05	-7.6e-05	2.6e-07
	(1.2e-04)	(7.3e-05)	(6.4e-05)	(1.1e-04)	(6.2e-05)	(5.4e-05)
initial land*program	0.005	-0.000	-0.000	-0.002	-0.000	0.001
	(0.003)	(0.002)	(0.002)	(0.002)	(0.004)	(0.005)
retail*program	-0.016	0.018	-0.006	0.042	-0.059*	-0.046
	(0.041)	(0.012)	(0.029)	(0.041)	(0.033)	(0.048)
livestock*program	0.110*	-0.011	-0.010	0.066	-0.040	-0.037
	(0.055)	(0.040)	(0.050)	(0.046)	(0.028)	(0.050)
constant	0.076	-0.094	0.004	0.050	0.500***	0.476***
	(0.086)	(0.070)	(0.099)	(0.062)	(0.119)	(0.114)
Observations	817	1035	1421	947	1701	1701
Number of Villages	13	13	14	14	14	14
R-squared	0.07	0.02	0.02	0.05	0.08	0.06

Robust standard errors in parentheses. Estimation uses year and village-level fixed effects.  
 \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

**Table 4—Test for Demand Endogeneity with Five-Year Credit Treatment Window**

	(1)	(2)	(3)	(4)	(5)	(6)
	newwalls	newroof	newfloor	newtoilet	newland	homeimprv
fyrcreditplus2	0.077*	0.018	0.085*	-0.031	0.028	0.025
	(0.044)	(0.044)	(0.040)	(0.036)	(0.125)	(0.051)
fyrcreditplus1	0.141	0.032	0.040	0.028	-0.008	0.047
	(0.119)	(0.037)	(0.034)	(0.045)	(0.080)	(0.073)
fyrcredit	0.059	-0.015	0.030	0.083	-0.006	0.085*
	(0.068)	(0.015)	(0.026)	(0.062)	(0.049)	(0.045)
fyrcreditminus1	0.039	-0.015	0.027	0.054	0.029	0.033
	(0.059)	(0.015)	(0.039)	(0.045)	(0.067)	(0.040)
fyrcreditminus2	-0.065	-0.026	0.000	0.013	-0.068**	-0.029
	(0.042)	(0.021)	(0.011)	(0.026)	(0.025)	(0.036)
noprogramminus1	-0.014	0.005	-0.019	-0.096	0.429	0.052
	(0.079)	(0.022)	(0.038)	(0.060)	(0.495)	(0.098)
noprogramminus2	0.052	0.079	0.035	0.060	0.039	0.047
	(0.077)	(0.061)	(0.050)	(0.069)	(0.042)	(0.051)
education father	-0.004	0.001	0.000	0.002	-0.009*	-0.006**
	(0.003)	(0.002)	(0.002)	(0.003)	(0.005)	(0.002)
education mother	0.003	0.003	-0.001	0.005***	0.002	0.002
	(0.005)	(0.002)	(0.003)	(0.002)	(0.005)	(0.002)
age of father	-0.009	0.000	0.001	-0.008	0.005	-0.001
	(0.008)	(0.003)	(0.002)	(0.010)	(0.006)	(0.006)
age father squared	9.8e-05	5.5e-06	-1.8e-05	1.2e-04	-5.8e-05	3.8e-05
	(9.3e-05)	(3.6e-05)	(2.5e-05)	(1.5e-04)	(7.0e-05)	(7.4e-05)
initial land (cuerdas)	-0.001	0.000	-0.002	-0.000	0.002	0.001
	(0.003)	(0.001)	(0.001)	(0.002)	(0.004)	(0.003)
retail	-0.010	0.008	-0.014	0.003	-0.044	-0.007
	(0.016)	(0.009)	(0.018)	(0.024)	(0.087)	(0.026)
livestock	0.058	0.032*	-0.001	0.027	-0.088	0.003
	(0.038)	(0.017)	(0.020)	(0.030)	(0.085)	(0.036)
Constant	0.164	-0.036	0.060	0.102	-0.024	0.097
	(0.195)	(0.066)	(0.054)	(0.128)	(0.088)	(0.119)
Observations	611	769	992	729	1185	1185
F-stat for Dem Endog.	0.11	1.46	0.05	0.16	0.91	0.94
p-value	0.747	0.250	0.821	0.698	0.358	0.349
Number of Villages	13	13	14	14	14	14
R-squared	0.09	0.04	0.02	0.07	0.02	0.03

Robust standard errors in parentheses. Estimation uses year and village-level fixed effects.  
\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

**Table 5A—Test for Take-up Effect of Microcredit**

	(1)	(2)	(3)	(4)	(5)	(6)
	newwalls	newroof	newfloor	newtoilet	newland	homeimprv
Credit Taken	0.066**	0.031	0.017*	0.000	-0.016	-0.041
	(0.032)	(0.024)	(0.010)	(0.043)	(0.025)	(0.029)
Constant	-0.032	-0.005	0.001	0.035	0.064***	0.129***
	(0.038)	(0.015)	(0.009)	(0.040)	(0.019)	(0.024)
Observations	1159	1359	1991	1298	2179	2359
No. of villages	13	13	14	14	14	14
R-squared	0.04	0.02	0.01	0.03	0.01	0.04

**Table 5B—Test for Take-up Effect of Microcredit with Take-up Restrictions**

<b>(Take-up lag after credit rollout ≤ 2 years.)</b>						
	(1)	(2)	(3)	(4)	(5)	(6)
	newwalls	newroof	newfloor	newtoilet	newland	homeimpr
Credit Taken	0.150***	0.002	0.030	0.047	-0.101	0.050
	(0.047)	(0.032)	(0.033)	(0.055)	(0.077)	(0.036)
Constant	-0.052	0.003	-0.031	0.053	0.139**	0.023
	(0.099)	(0.037)	(0.034)	(0.068)	(0.054)	(0.031)
Observations	366	431	623	404	670	726
No. of villages	11	11	12	12	12	12
R-squared	0.14	0.05	0.02	0.05	0.02	0.05

<b>(Take-up lag after credit rollout ≤ 1 year.)</b>						
	(1)	(2)	(3)	(4)	(5)	(6)
	newwalls	newroof	newfloor	newtoilet	newland	homeimpr
Credit Taken	0.113	0.028	-0.009	0.011	-0.089	0.080**
	(0.078)	(0.034)	(0.031)	(0.069)	(0.132)	(0.035)
Constant	0.029	-0.023	0.014	0.124	0.090	-0.030
	(0.167)	(0.039)	(0.030)	(0.125)	(0.123)	(0.060)
Observations	242	254	377	234	410	439
No. of villages	10	10	11	11	11	11
R-squared	0.15	0.08	0.04	0.08	0.03	0.07

<b>(Take-up lag after credit rollout = 0 years.)</b>						
	(1)	(2)	(3)	(4)	(5)	(6)
	newwalls	newroof	newfloor	newtoilet	newland	homeimpr
Credit Taken	0.092	-0.013	0.039	0.005	-0.286	0.022
	(0.121)	(0.029)	(0.049)	(0.093)	(0.336)	(0.089)
Constant	0.102	0.028	-0.027	0.131	0.287	0.066
	(0.284)	(0.035)	(0.060)	(0.169)	(0.302)	(0.107)
Observations	120	123	212	148	214	235
No. of villages	8	11	11	10	11	11
R-squared	0.28	0.11	0.10	0.12	0.08	0.13

Robust standard errors in parentheses  
 \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

**Table 6A—Five-Period Treatment Window with F-tests**

	(1)	(2)	(3)	(4)	(5)	(6)
	newwalls	newroof	newfloor	newtoilet	newland	homeimprov
fyrcreditplus2	0.080*	0.025	0.074**	-0.031	-0.009	0.032
	(0.046)	(0.045)	(0.036)	(0.039)	(0.025)	(0.049)
fyrcreditplus1	0.144	0.039	0.037	0.027	0.028	0.054
	(0.119)	(0.039)	(0.032)	(0.043)	(0.045)	(0.072)
fyrcredit	0.062	-0.008	0.031	0.082	0.024	0.092*
	(0.067)	(0.016)	(0.027)	(0.061)	(0.018)	(0.047)
fyrcreditminus1	0.039	-0.009	0.033	0.037	0.010	0.046
	(0.050)	(0.014)	(0.029)	(0.035)	(0.018)	(0.035)
fyrcreditminus2	-0.047	-0.005	0.008	0.029	-0.015	-0.015
	(0.036)	(0.025)	(0.015)	(0.039)	(0.017)	(0.034)
education father	-0.004	0.001	0.001	0.002	-0.003**	-0.006**
	(0.003)	(0.001)	(0.001)	(0.003)	(0.001)	(0.002)
education mother	0.003	0.003	-0.001	0.005***	0.001**	0.002
	(0.005)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
age of father	-0.009	0.000	0.002	-0.009	0.001	-0.001
	(0.008)	(0.003)	(0.003)	(0.010)	(0.002)	(0.006)
age father squared	2.6e-05	6.4e-07	-1.8e-05	1.2e-04	-1.2e-05	4.9e-06
	(3.3e-05)	(2.1e-05)	(2.7e-05)	(1.5e-04)	(2.1e-05)	(7.3e-05)
initial land (cuerdas)	-0.001	-0.000	-0.001	-0.000	-0.000	0.001
	(0.003)	(0.001)	(0.001)	(0.002)	(0.001)	(0.003)
retail	-0.010	0.006	-0.013	0.001	0.006	-0.008
	(0.016)	(0.009)	(0.014)	(0.025)	(0.022)	(0.025)
livestock	0.057	0.031*	0.003	0.025	-0.005	0.003
	(0.037)	(0.017)	(0.017)	(0.031)	(0.022)	(0.035)
constant	0.168	-0.036	0.050	0.109	-0.005	0.099
	(0.193)	(0.066)	(0.062)	(0.128)	(0.035)	(0.116)
Observations	611	769	1053	729	1185	1185
Number of local	13	13	14	14	14	14
F-statistic: 2 Post-Treatment vs. .2 Pre-Treatment	<b>4.73**</b>	1.26	<b>5.25**</b>	1.11	0.47	0.28
p-value	<b>0.050</b>	0.2814	<b>0.039</b>	0.312	0.503	0.606
R-squared	0.09	0.03	0.02	0.05	0.01	0.03

Robust standard errors in parentheses. Estimation uses year and village-level fixed effects.  
\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Figure 1

Change in Probability of New Walls  
5-year Credit Window

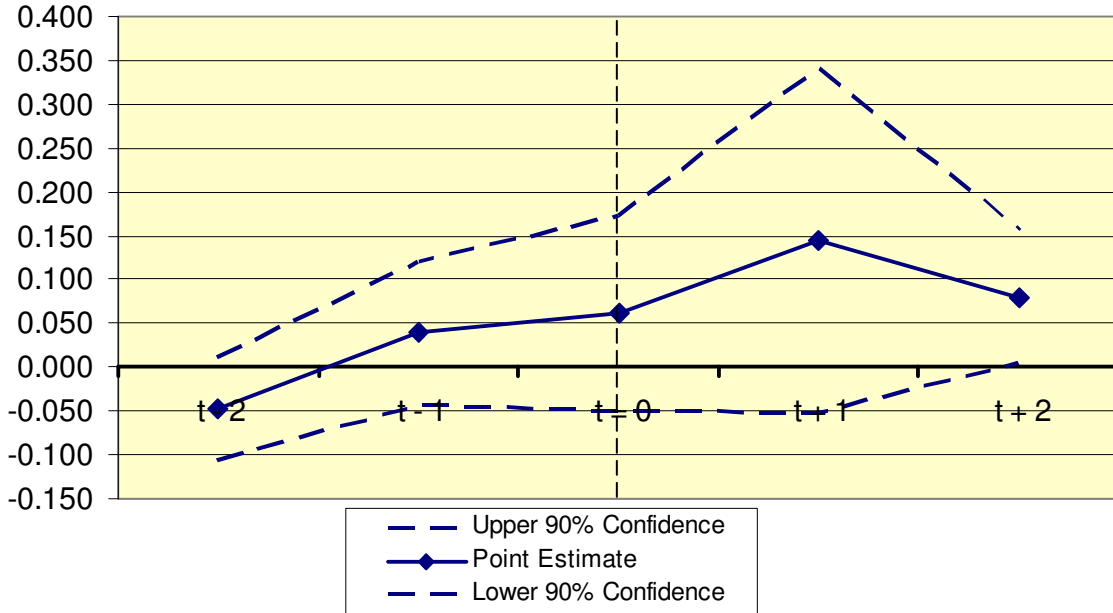


Figure 2

Change in Probability of New Roof:  
5-year Credit Window

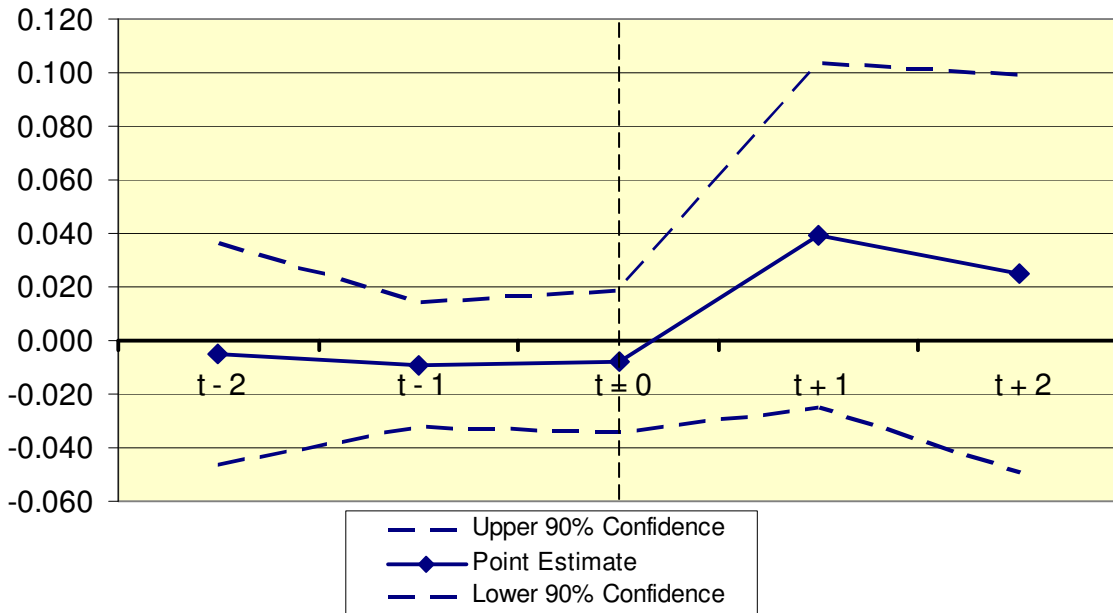


Figure 3

Change in Probability of New Floor  
5-year Credit Window

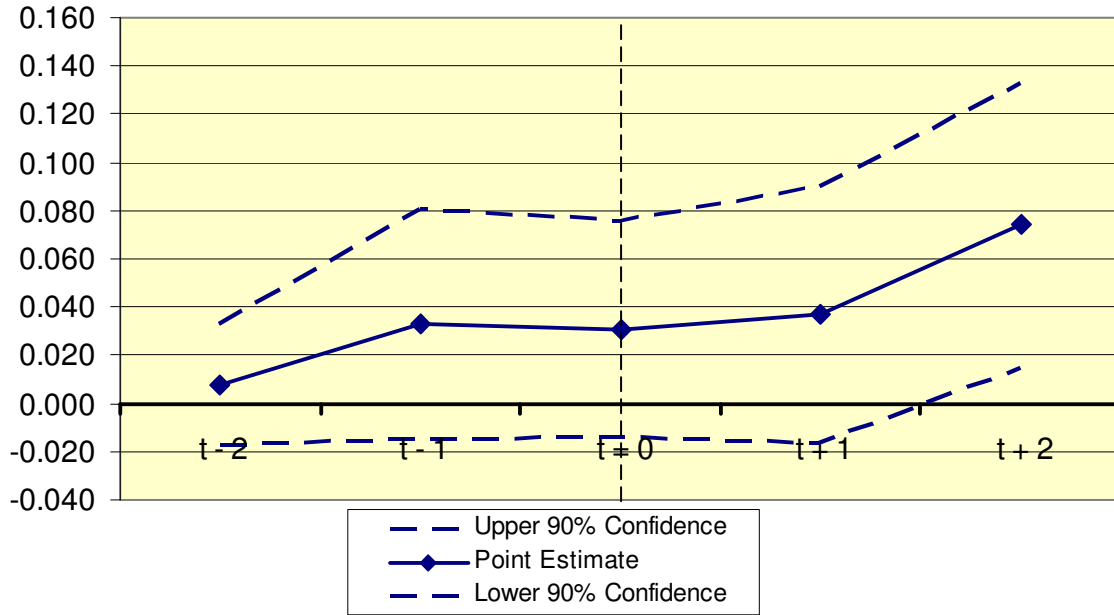


Figure 4

Change in Probability of New Toilet:  
5-year Credit Window

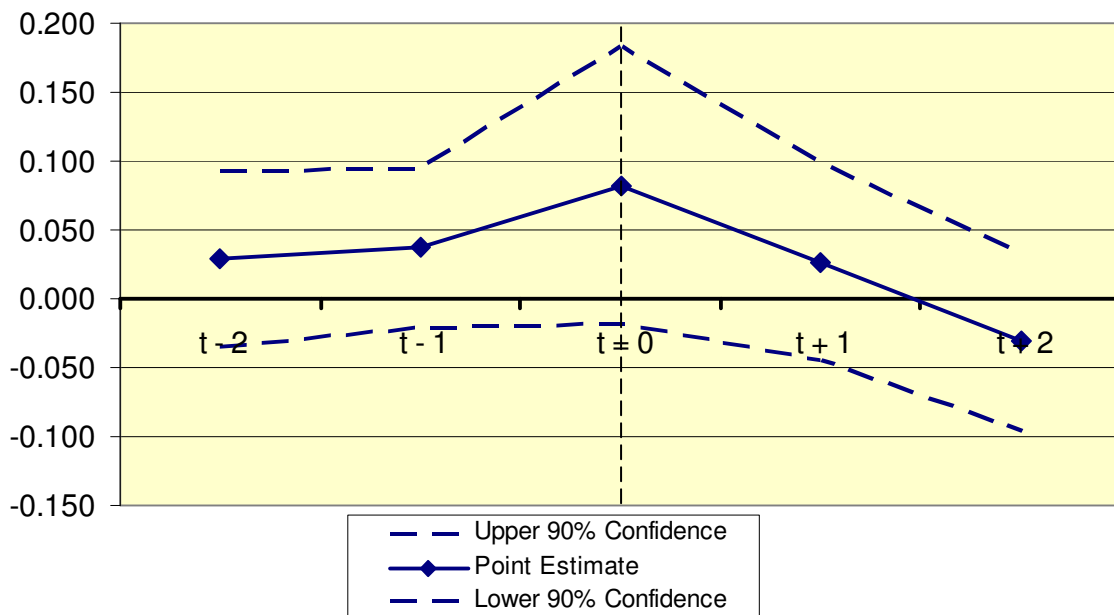


Figure 5

Change in Probability of New Land Purchase:  
5-year Credit Window

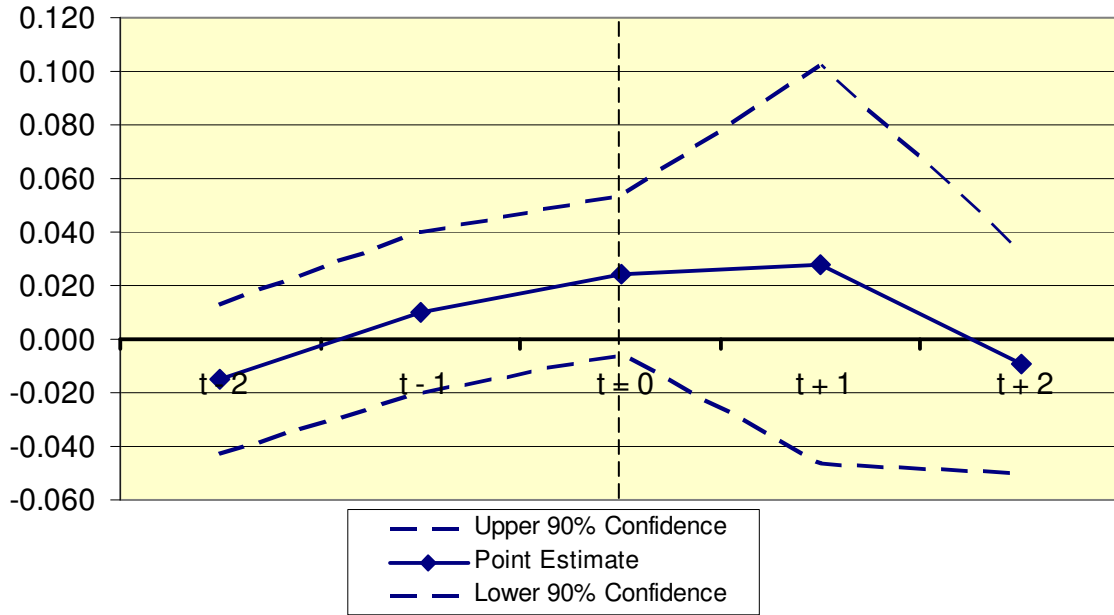


Figure 6

Change in Probability of Home Improvement:  
5-year Credit Window

